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Identifying SVARs from sparse narrative instruments: dynamic effects of U.S. macroprudential policies

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Abstract

We study the identification of policy shocks in Bayesian proxy VARs for the case that the instrument consists of sparse qualitative observations indicating the signs of certain shocks. We propose two identification schemes, i.e. linear discriminant analysis and a non-parametric sign concordance criterion. Monte Carlo simulations suggest that these provide more accurate confidence bounds than standard proxy VARs and are more efficient than local projections. Our application to U.S. macroprudential policies finds persistent effects of capital requirements and mortgage underwriting standards on credit volumes and house prices together with moderate effects on GDP and inflation.

JEL classification: C32, E44, G38.

Keywords: Bayesian Proxy VAR, Discriminant Analysis, Sign Concordance, Capital Requirements, Mortgage Underwriting Standards.

Non-technical summary

Since the 2008 Global Financial Crisis policy-makers have developed new macroprudential regulatory policies, targeted at dampening cyclical fluctuations in credit and house prices. Studies assessing the effectiveness of the related policy instruments must rely on historical, qualitative data about the timing and direction of related supervisory interventions. Typically, these data are included as regressors in cross-country panel regressions to assess the effects of interventions on credit volumes and house prices. Studies thereby remain silent on issues such as transmission lags of policy interventions and their cost in terms of GDP and inflation.

In this paper, we explore the dynamic effects of macroprudential policy interventions from proxy vector autoregressions (VARs). Proxy VARs model the joint dynamics of the series of interest and identify the dynamic effects of policy interventions from using a respective indicator as an instrument. We provide a methodological contribution by adapting the proxy VAR approach to the case of sparse qualitative indicators, as faced with macroprudential policies, based on linear discriminant analysis and on the sign concordance of shocks with the indicators. A simulation study shows that the combination of the two criteria provides more accurate confidence bounds than existing versions of the proxy VAR approach and is more robust to observation errors.

We then study the effects of U.S. policy interventions related to capital requirements and mortgage underwriting standards over the period of 1956 to 2016. We find highly persistent effects of both types of policy interventions on credit volumes and less persistent and more moderate effects on GDP, inflation, and corporate bond spreads. Shocks to capital requirements impact on credit to non-financial corporations, while household credit and house prices remain unaffected, reflecting a shift towards lower risk weights in bank credit portfolios. By contrast, mortgage underwriting standards affect both types of credit and have a pronounced impact on house prices.

These results point to long lags in the transmission of macroprudential policies indicating a need for rule-based forward-looking policies. They also suggest that static panel regressions may underestimate the effects of macroprudential policies.

1 Introduction

In this paper, we explore a Bayesian proxy VAR approach to estimating the aggregate dynamic effects of macroprudential policy interventions. We provide a methodological contribution by adapting a Bayesian proxy VAR to the case of sparse qualitative instruments. We then study the effects of post-war U.S. policy interventions related to capital requirements and mortgage underwriting standards based on the narrative indicators of Elliot, Feldberg, and Lehnert (2013).

Macroprudential policies have been defined after the 2008 Global Financial Crisis to stem financial instabilities and to dampen credit and house price cycles. Due to the novelty of the policies, the growing literature on assessing their aggregate effects must rely on related historical supervisory regulatory interventions, which are diverse and scattered across countries and over time, impeding the construction of quantitative measures of the policy stance. Most studies therefore use purely qualitative indicators, which they include in cross-country panel regressions to assess the effects of interventions on credit volumes and house prices.[1] Galati and Moessner (2016) note that these studies thereby remain silent on issues such as policy transmission lags and macroeconomic implications. Only a few recent papers apply local projection methods (Richter, Schularick, and Shim, 2018; Eickmeir, Kolb, and Prieto, 2018).

An alternative approach to estimating dynamic effects is proxy VARs that identify policy shocks as a linear combination of the VAR residuals by using the narrative indicator as an instrument. One particular advantage of proxy VARs is their robustness to measurement error in the indicator. Sparse instruments are common in applications.[2] While frequentist versions account for sparsity by using bootstrap methods to obtain confidence bounds, the Bayesian literature on proxy VARs has so far ignored the implications of sparse or qualitative instruments (Arias, Rubio-Ramirez, and Waggoner, 2018b; Caldara and Herbst, 2019; Giacomini, Kitagawa, and Read, 2019). Bayesian VARs have yet several potential advantages when it comes to assessing macroprudential policies, including long lag structures or panel VAR extensions.

[1]See Galati and Moessner (2016) and Boissay et al (2019) for reviews of the empirical literature.
[2]See e.g. Mertens and Ravn (2013), Mertens and Montiel Olea (2018).

We therefore study two identification schemes that provide proper inference for sparse qualitative instruments in Bayesian proxy VARs. First, we show that a minor modification of the standard approach attains an interpretation as linear discriminant (*DC*) analysis, which is applicable to qualitative data. Second, we use a stochastic version of the narrative sign restrictions proposed by Ludvigson, Ma, and Ng (2017) and Antolin-Diaz and Rubio-Ramirez (2018). This amounts to accepting only those shock decompositions, for which the signs of the resulting policy shocks correspond to the narrative indicator for a sufficiently high number of events. By using a prior distribution for sign concordance our version is suitable for a higher number of events.

Monte Carlo simulations indicate that the *DC* regression is as efficient as the standard proxy VAR but provides more accurate confidence bounds than frequentist bootstrap methods. The *SC* prior is less efficient if the narrative indicator is well defined, but provides useful support to the *DC* regression in case of observation errors, which are a pervasive issue in application. In this case, we find a combination of the *DC* regression with the *SC* prior to perform best. Generally, the efficiency losses from using qualitative instead of quantitative instruments remain moderate. Further, local projections and recursive VARs turn out clearly less efficient than proxy VARs.

We then explore post-war U.S. policy interventions related to capital requirements and mortgage underwriting standards, building on the dataset of Elliot, Feldberg, and Lehnert (2013). The institutional features of the U.S. policy framework suggest that these interventions have been exogenous to contemporaneous macroeconomic shocks. While they partly reflect a lagged response to macro-financial developments, our two indicators pass the invertibility test of Plagbørg and Wolf (2018) suggesting that our VAR including seven variables accounts for lagged dependencies.

We find significant and highly persistent effects of both types of policy interventions on credit volumes and moderate effects on GDP, inflation, and corporate bond spreads. Capital requirements impact on credit to non-financial corporations, but leave household credit and house prices unaffected, reflecting a shift towards lower risk weights in bank credit portfolios. Mortgage underwriting standards affect both types of credit and house prices, and have a persistent effect on economic activity. Altogether, our estimates point to long lags in the transmission of macroprudential policies. Panel regressions inspecting the contemporaneous response of annual growth in credit and house prices therefore very likely understate their total effects. Our results also relate to the literature on credit supply shocks (Gilchrist and Zakrajsek, 2012) and government mortgage purchases (Fieldhouse, Mertens, and Ravn, 2017), and underpin the role of collateral constraints in generating the high persistence of leverage cycles found by Claessens, Kose, and Terrones (2015) and Rünstler and Vlekke (2018).

The remainder of the paper is organised as follows. Section 2 introduces our DC and SC restrictions. Sections 3 and 4 present the Monte Carlo simulation exercise and the application to U.S. macroprudential policies, respectively. Section 5 concludes.

2 VAR Identification from Qualitative Instruments

Consider the reduced form VAR for $n \times 1$ vector y_t over periods $t = 1, \dots, T$,

$$y_t = c + \sum_{s=1}^{p} B_s y_{t-s} + u_t, \tag{1}$$

Denote $B_+ = (c, B_1, \dots, B_p)$ and $Y^T = (y_1^T, \dots, y_T^T)$ and assume that the posterior of $\mathrm{vec}(B_+, \Sigma)|Y$ is known. Residuals u_t embed the impact Γ of an unobserved scalar policy shock θ_t,

$$u_t = u_t^+ + \Gamma\theta_t, \tag{2}$$

where Γ is an $n \times 1$ vector and $\theta_t \sim N(0, \sigma_\theta^2)$. Further assume that $n \times 1$ vector u_t^+ is distributed with $u_t^+ \sim N(0, \Sigma^+)$ and let $\mathbb{E}u_t^+\theta_t = 0_n$, where 0_n is an $n \times 1$ zero vector.

The VAR residuals have a structural representation $u_t = A_0^{-1}\epsilon_t$ with $\mathbb{E}\epsilon_t\epsilon_t^T = I_n$, which isolates the impact of the policy shock in the first element of the vector of structural shocks ϵ_t,

$$y_t = c + \sum_{s=1}^{p} B_s y_{t-s} + A_0^{-1}\left(\epsilon_t^+ + \begin{bmatrix} \gamma \\ 0_{n-1} \end{bmatrix} \theta_t\right), \tag{3}$$

with scalar $\gamma > 0$. This representation may be achieved along the lines of Arias et al (2018a) by setting $A_0^T = A_* Q$, where $\Sigma^{-1} = A_* A_*^T$ from a Choleski decomposition and orthogonal matrix Q is chosen such that $A_0^{-1}\Gamma = (\gamma, 0_{n-1}^T)^T$.

Let α^T be the first row of matrix A_0 and $\epsilon_{1,t}$ the first element of vector ϵ_t, such that

$$\alpha^T u_t = \epsilon_{1,t} = \epsilon_{1,t}^+ + \gamma\theta_t.$$

The key assumption of this section is that the econometrician only observes the sign $z_t = \text{sign}(\theta_t)$ of the policy shock and that she does so only for a limited number $m < T$ of periods. We set $z_t = 0$ otherwise. The aim is to identify α^T by means of the narrative instrument z_t in order to estimate the impulse response (IRF) of y_t to θ_t. Note that the scale of policy shock θ_t is not identified under this assumption. We normalize it to $\sigma_\theta^2 = \pi/2$, which implies $\mathbb{E}|\theta_t| = 1$ (Tsagris et al, 2014). We will refer to parameter γ as the mean absolute policy impact.

The model is a special case of the proxy VAR approach (Stock and Watson, 2012; Mertens and Ravn, 2013), which starts from the assumption that instrument z_t is correlated with $\epsilon_{1,t}$ but orthogonal to the remaining innovations, $\mathbb{E}\epsilon_{j,t}z_t = 0$ for $j > 1$. An estimate of α is obtained by exploiting these $n-1$ zero moment conditions, as from a regression of z_t on residuals u_t. In practice, instrument z_t often contains only a small number of non-zero observations. This implies a non-standard distribution of the moment conditions and thereby creates issues with inference on IRF confidence bounds.[3] Frequentist versions of proxy VARs rely on bootstrap methods for this purpose. Jentsch and Lunsford (2016) show that standard bootstraps may substantially underestimate confidence bounds with sparse instruments and propose a modified block bootstrap, while Montiel Olea, Stock, and Watson (2018) discuss bootstrap methods that are robust to weak identification.

Bayesian approaches to proxy VARs have so far maintained standard distributional assumptions that are not applicable to sparse instruments. Caldara and Herbst (2019) and Giacomini et al (2019) assume a normal distribution of the residual ζ_t in the proxy regression $z_t = \alpha^T u_t + \zeta_t$. To cope with weak instrument concerns the former paper imposes a prior on the correlation of $\epsilon_{1,t}$ with z_t, which reflects beliefs on the reliability of the instrument. Arias et al (2018b) include z_t in the vector of endogenous variables of the VAR and discuss the case of a higher number of instruments. They implement

[3] This applies in particular to studies on fiscal policies (e.g. Favero and Giavazzi, 2012; Mertens and Ravn, 2013; Mertens and Montiel Olea, 2018). Studies identifying monetary policy shocks from high-frequency financial market data (e.g. Gertler and Karadi, 2016; Jarocinski and Karadi, 2019) face more regular instruments.

the moment conditions $\mathbb{E}\epsilon_{j,t}z_t = 0$ as deterministic zero restrictions in the posterior of matrix A_0 and thereby ignore uncertainty in the conditions.

With a qualitative instrument, identification effectively becomes a classification problem and amounts to exploiting the shifts in the conditional distribution of $\alpha^T u_t$ in dependence on z_t. We explore two identification schemes to obtain posterior distributions of α. First, we use linear discriminant (DC) analysis to maximize a function of the difference in the conditional means of $\alpha^T u_t$. One version of DC analysis can be implemented as a regression, which turns out to result only in a minor modification of the standard proxy VAR approach, but is subject to standard inference and allows for sampling of $\alpha|B_+$ via a conjugate prior. The second, non-parametric, approach is based on the sign concordance (SC) of $\alpha^T u_t$ with non-zero observations in z_t. We draw from an uninformative prior of α and accept draws that achieve a sufficient degree of sign concordance based on a prior weighting scheme.

2.1 Discriminant Regression

The parametric approach exploits the shifts in the conditional distributions of $\alpha^T u_t = \epsilon_{1,t}$ for different values of z_t. Our distributional assumptions imply that $\epsilon_{1,t}|z_t \neq 0 \sim N(\gamma z_t, \alpha^T\Sigma^+\alpha)$ and $\epsilon_{1,t}|z_t = 0 \sim N(0, \alpha^T\Sigma^+\alpha)$. At the same time, the distributions of the remaining shocks are independent of z_t, i. e. $\epsilon_{j,t}|z_t = k \sim N(0,1)$ for $j > 1$ and $k = 0, 1$. This suggests estimating α by maximizing a function of the differences in the conditional means of $\alpha^T u_t$, which amounts to discriminant analysis.

Discriminant analysis is, for instance, discussed by Maddala (2013:79ff). The general objective is to estimate function $\psi(u_t)$ to predict a dichotomous variable z_t from the rule $\hat{z}_t = 1$ if $\psi(u_t) > 0$ and $\hat{z}_t = 0$ otherwise. While z_t can take the values 0, 1, and -1 in our application, the symmetry of the latter two cases allows for reducing the estimation problem to the dichotomous case. This is achieved by considering $\delta_t z_t$ and $\delta_t u_t$, where $\delta_t = -1$ if $z_t = -1$ and $\delta_t = 1$ otherwise. Given that residuals u_t^+ are normally distributed, the optimal discriminant function is linear.

Under a further assumption on the relative costs of misclassification, discriminant analysis can be implemented from a linear regression. The maximum likelihood estimate of vector α can be obtained up to scale from the OLS estimate of the regression

$$z_t = a_0\delta_t + a^T u_t + \zeta_t. \tag{4}$$

Coefficients a maximize the objective function $(a^T\Sigma^+ a)^{-1}(a^T(\overline{u}_{z,1} - \overline{u}_{z,0})^2$, i.e. the squared difference in conditional means $a^T\overline{u}_{z,k} = a^T \sum_{|z_t|=k}(\delta_t u_t)$, scaled by the variance within groups. The derivation of equation (4) is discussed in Annex A.1.[4]

Crucially, the discriminant regression is subject to standard inference (Maddala, 2013:18ff). In a Bayesian framework this allows for sampling from the conditional posterior of α based on a Normal-Gamma conjugate prior. We assume an uninformative prior for a together with a Jeffrey prior for the residual standard error σ_ζ. This gives the conditional posterior as $a|B_+, \Sigma, \sigma_\zeta, Y, Z \sim N(\hat{a}, \sigma_\zeta^2 S_u^{-1})$ and $\sigma_\zeta|B_+, \Sigma, Y, Z \sim IG(\hat{\sigma}_\zeta, T - n - 1)$, where $\hat{a}$ and $\hat{\sigma}_\zeta$ are obtained from the OLS estimate of equation (4) and $S_u = \Sigma_{t=1}^T u_t u_t^T$.

Since the posterior $p(B_+, \Sigma|Y)p(a|B_+, \Sigma, Y, Z)$ is factorized such that parameters (B_+, Σ) are independent of a, draws can be obtained in two steps.

1. Draw from the posterior $p(B_+, \Sigma|Y)$ of the reduced-form VAR (1).

2. Draw from the conditional posteriors of $\sigma_\zeta|B_+, \Sigma, Y, Z$ and $a|B_+, \Sigma, \sigma_\zeta, Y, Z$ and obtain $\alpha = (a^T S_u a)^{-1} a$ in representation (3) such that $\text{var}(\alpha^T u_t) = 1$.

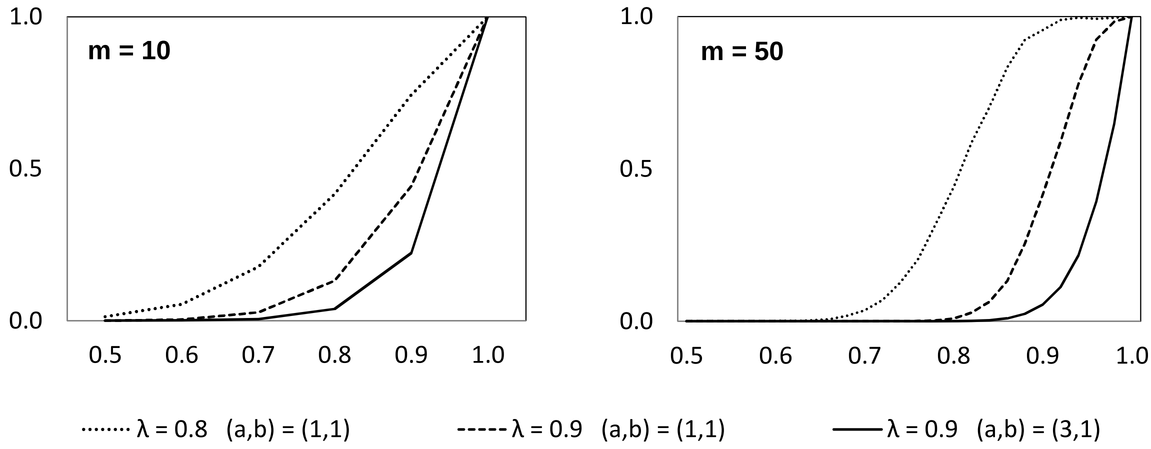
2.2 Sign Concordance

Ludvigson et al (2017) and Antolin-Diaz and Rubio-Ramirez (2018) propose identification based on narrative information about single events and argue that respective restrictions on the sign of shocks in individual periods add important information to the shock decomposition. With the sign concordance (SC) prior we generalize this idea to a higher number of events, while abandoning the assumption of perfect

[4]The DC regression can be derived as the solution to the classification problem under the loss function $mC_1 = (T - m)C_0$, where C_k is the cost of misclassifying an observation under $\delta_t z_t = k$. Uner small m, this imposes a high cost of misclassifying non-zero z_t, which is desired.

information about the signs of individual shocks. Note that equation (3) implies that $\mathrm{sign}(\epsilon_{1,t}) = z_t$ may not hold for a certain number of events due to innovations $\epsilon_{1,t}^{+}$.[5]

Figure 1: Beta-Binomial Priors for Sign Concordance



The graphs show densities $f(\varphi; m, \Lambda, a, b)$ of beta-binomial distributions for $m = 10$ and $m = 50$.

We achieve identification from the restriction that the signs of shocks $\epsilon_{1,t} = \alpha^T u_t$ correspond to the narrative indicator z_t for a sufficiently high number of non-zero observations in z_t. Define sign concordance φ as the share of instances, for which the sign of $\epsilon_{1,t}$ coincides with z_t,

$$\varphi(B_+, \alpha, Y, Z) = m^{-1} \sum_{z_t \neq 0} \mathbf{I}(\epsilon_{1,t} z_t > 0). \tag{5}$$

where $\mathbf{I}()$ denotes the indicator function and m is the number of non-zero observations in z_t. We combine this with a weighting scheme to accept draws of α dependent on the value of φ. We base our acceptance criterion on a certain prior belief $0 < \lambda \leq 1$ about the probability that $\epsilon_{1,t}$ has the correct sign for a given event, $p(\epsilon_{1,t} z_t > 0 | z_t \neq 0) = \lambda$. Given the independence of $\epsilon_{1,t}$ over time, the number of events with correct sign then follows a binomial distribution,

$$p(m\varphi | \alpha, \lambda, B_+, Y, Z) = f_z(m\varphi; m, \lambda). \tag{6}$$

Clearly, the prior belief on λ should be set to support acceptance of high values of φ. A flexible choice is a beta-distribution $\lambda \sim \beta(a, b)$ over support $[\Lambda, 1]$ with $\Lambda > 0.5$.

[5]Innovations $\epsilon_{1,t}^{+}$ may, for instance, represent market-based shocks of a similar type as the policy innovations that occasionally override the latter.

Figure 1 shows examples of the resulting beta-binomial prior $f(\varphi; m, \Lambda, a, b)$ for φ for different values of m, Λ, and (a, b). The benchmark case of a uniform distribution, $a = b = 1$, creates acceptance weights with a smooth threshold for φ around Λ, while more sharply increasing acceptance weights may be created from $a > 1$.

The posterior $p(B_+, \Sigma, \alpha, \lambda|X, Z)$ is no longer separable between B_+, Σ and α, λ. We follow Arias et al (2018a) and draw from the posterior by means of rejection sampling.

1. Draw from the posterior $p(B_+, \Sigma|Y)$ of the reduced-form VAR (1).
2. Obtain an uninformative draw of $\alpha|B_+, \Sigma, Y$. We specify $\alpha = A_* q_1$, where A_* is the Choleski decomposition of Σ and q_1 is the first column of a random draw from the Haar measure of orthogonal matrices. This is obtained as $q_1 = x/||x||$ from a random draw of vector $x \sim N(0, I_n)$.
3. Draw from the prior of λ and accept the draw with probability $f_z(m\varphi; m, \lambda)$.

2.3 Further Remarks

DC and *SC* restrictions may be combined with each other, in which case the latter attains an interpretation as reliability prior, which regulates the informativeness of the proxy for estimation. Such reliability prior has been proposed by Caldara and Herbst (2019) for a proxy VAR with standard distributional assumptions to give higher weight to draws of α with high correlation of shocks $\epsilon_{1,t}$ with instrument z_t. In the case of a qualitative instrument, a measure of instrument reliability is provided by the sign concordance statistics. The combination of the *DC* regression with the *SC* prior can be implemented from drawing $\alpha|B_+, Y, Z$ as described in section 2.1 and a subsequent rejection sampling step based on $f_z(m\varphi; m, \lambda)$ as described in section 2.2.

Both methods also provide an estimate of parameter γ, the mean absolute impact of policy shocks in equation (3). The normalizations $\text{var}(\alpha^T u_t) = \mathbb{E}|\theta_t| = 1$ imply that $\mathbb{E}\epsilon_{1,t}|z_t = \gamma\mathbb{E}(|\theta_t|z_t)|z_t = \gamma z_t$. Using an uninformative prior $p(\gamma)$, posterior draws of $\gamma|B_+, \alpha, Y, Z$ can be obtained from the conditional mean of $\epsilon_{1,t}$ for $z_t \neq 0$,

$$\hat{\gamma} = m^{-1} \sum_{t=1}^{T} (\alpha^T u_t) z_t. \tag{7}$$

Further, Annex A.2 embeds DC and SC restrictions in the framework of Arias et al (2018a). They may therefore be combined with other types of restrictions supported by the latter, such as sign on IRFs to any shock $\epsilon_{j,t}$, $j = 1, \ldots, n$.

One alternative to proxy VARs is local projections (LP), which amount to estimating the h-step ahead response of $y_{i,t}$ to z_t directly from coefficient $\gamma_{i,h}$ in the regression

$$y_{i,t+h} = \gamma_{i,h} z_t + c_h + \sum_{s=1}^{p} \beta_{i,s,h}^T y_{t-s} + u_{i,t,h}, \tag{8}$$

with $h \geq 0$ (Jorda et al, 2015). LP are asymptotically less efficient than proxy VARs. While Kilian and Kim (2013) provide some corresponding simulation-based evidence for small samples, Plagborg-Møller and Wolf (2019) argue that relative efficiency in finite samples may go either way and propose a recursive VAR implementation of LP by including z_t in the VAR ordered first and performing a Choleski decomposition. Yet with a qualitative regressor z_t that measures policy shocks with an error a regression-based approach may lose efficiency compared to the proxy VAR.[6]

While local projections and the recursive VAR are not subject to invertibility issues, Stock and Watson (2018) show that the requirements to the instrument z_t are in the end equivalent for proxy VARs and LP. Both methods require z_t to be exogenous to contemporaneous shocks other than policy shock θ_t. LP further require exogeneity of z_t to past shocks, after accounting for the control variables included in the regression. Proxy VARs instead require the invertibility condition that the shock is spanned by the VAR residuals as in equation (2). Invertibility of the VAR is yet equivalent to dynamic exogeneity: both depend on whether the lagged determinants of policy interventions are included in the VAR (Forni and Gambetti, 2014; Lütkepohl, 2014). Consequently, dynamic exogeneity for LP requires including the same set of control variables in equation (8) as are needed for invertibility of the proxy VAR. The invertibility condition is testable from the requirement that the indicator does not Granger-cause the variables included in the VAR (Plagborg-Møller and Wolf, 2018).

[6] Several papers (Stock and Watson, 2018; Ramey and Zubairy, 2017; Mertens and Montiel Olea, 2018) use a variant, where z_t is used as an instrument for a quantitative policy variable in equation (8). In our case, the quantitative policy variable is lacking and z_t is used directly as a regressor.

3 Sparse Policy Interventions: a Monte Carlo Study

This section presents a set of Monte Carlo simulations to compare the estimates of IRFs from DC and SC restrictions with frequentist proxy VARs and local projections. We inspect the bias and uncertainty of IRF estimates from the various models together with the accuracy of confidence bounds. We also study potential biases arising from observation error in z_t and from lagged dependency of z_t on the variables included in the VAR. We use the data generating process

$$\begin{bmatrix} y_{t,1} \\ y_{t,2} \end{bmatrix} = B_1 \begin{bmatrix} y_{t-1,1} \\ y_{t-1,2} \end{bmatrix} + A_0^{-1} \left(\begin{bmatrix} \theta_t \\ 0 \end{bmatrix} + \begin{bmatrix} \varepsilon_{t,1}^{+} \\ \varepsilon_{t,2}^{+} \end{bmatrix} \right), \tag{9}$$

with $\epsilon_t^{+} \sim N(0, 10^{-2} I_2)$. We choose matrix B_1 to generate cyclical fluctuations with a length of 32 quarters and matrix A_0 to impose a correlation of 0.3 among residuals and achieve IRFs of convenient shape. Policy interventions θ_t are given by the rule

$$\begin{aligned} \theta_t^* &= \omega y_{t-1,2} + \nu_t \\ \theta_t &= -\mathbf{I}(\theta_t^* \geq \overline{\theta}) \xi_t. \end{aligned}$$

The policy rule generates rare policy interventions that arise from both exogenous policy shocks ν_t and the lagged dependency of the policy target θ_t^* on the past state of the system. The policy-maker intervenes only in case that the policy target θ_t^* exceeds a certain threshold $\overline{\theta}$. The size of interventions θ_t is random, drawn from a lognormal distribution $\ln(\xi_t/\overline{\xi}) \sim N(\sigma_\theta, \sigma_\theta^2/2)$ with $\overline{\xi} = 0.01$ and dispersion σ_θ. The econometrician observes $z_t = \text{sign}(\theta_t)$, possibly subject to error, which we specify below. Further details of the DGP are given in Annex A.3.

We compare seven models. We consider the DC regression, the SC criterion based on a uniform prior for λ over interval $[0.9, 1]$, and the combination of DC with the SC prior (model DSC). As benchmarks, we use two standard proxy VARs (pV) using either the true policy shock θ_t or the narrative z_t as instrument. Further, we inspect local projections (LP) as from equation (8) and the recursive VAR (rV) proposed by Plagborg-Møller and Wolf (2019). For each draw of the DGP (9), we obtain the central estimate of IRFs together with its confidence bounds, based either on the model posteriors (DC, SC, and DSC), the bootstrap of Montiel, Stock Watson (2018) for the two proxy VARs and LP, and a standard bootstrap for model rV.[7]

For our baseline simulations we let $\omega = 0$. We set the number of policy interventions to either $m = 10$ or $m = 20$ and their dispersion to $\sigma_\theta = 0.005$ or $\sigma_\theta = 0.01$. To detect potential biases, we consider only positive interventions. Table 1 shows three statistics on the IRF for $y_{1,t}$, i.e. the bias of the central estimate, the [0.1,0.9] interquantile difference of its distribution as a measure of its uncertainty, and the width of the corresponding [0.1,0.9] confidence bounds. Figure A.2 plots the IRFs.

We find, first, that the efficiency losses from using z_t in place of the true policy shock θ_t remain moderate, but increase with the dispersion of θ_t. The comparison of simulations (1) and (2) shows that higher σ_θ increases the uncertainty of estimates from model pV_z, but leaves the one of model pV_θ, based on instrument θ_t, unaffected.

Second, model DC gives more accurate confidence bounds than the pV_z bootstrap. Central estimates from the two models are similar by construction and so is their uncertainty. Confidence bounds are overestimated by both models but more so by the pV_z bootstrap. The combination of DC with the SC prior appears to further correct for this tendency, as the reliability prior restricts the parameters of the reduced form VAR. Model DSC tends to underestimate confidence bounds at horizon 0, but provides more accurate estimates than model DC at horizon 4. The SC prior by itself performs worse, as the uncertainty of estimates is higher across all simulations. Similarly, the combination of model DC with the SC prior entails small efficiency losses. We find that these patterns hold for a wide range of values of m, σ_θ and $\overline{\xi}$.

Third, local projections and the recursive VAR are clearly less efficient than the above models. For all simulations, the uncertainty of estimates at horizon 0 is about twice as large compared to model DC, while the relative efficiency of LP decreases further at higher horizons (see Fig 2). We note that their relative performance may yet improve in case of invertibility issues. Fourth, simulation (4) confirms that the dependency of policy events on lagged dependent variables of the VAR has negligible effects.

[7] For proxy VARs and LP we build on the replication files of Mertens and Montiel Olea (2018). We also used the bootstrap by Jentsch and Lunsford (2016) with very similar results. For models DC, SC, and DSC we use an uninformative prior for the reduced form VAR.

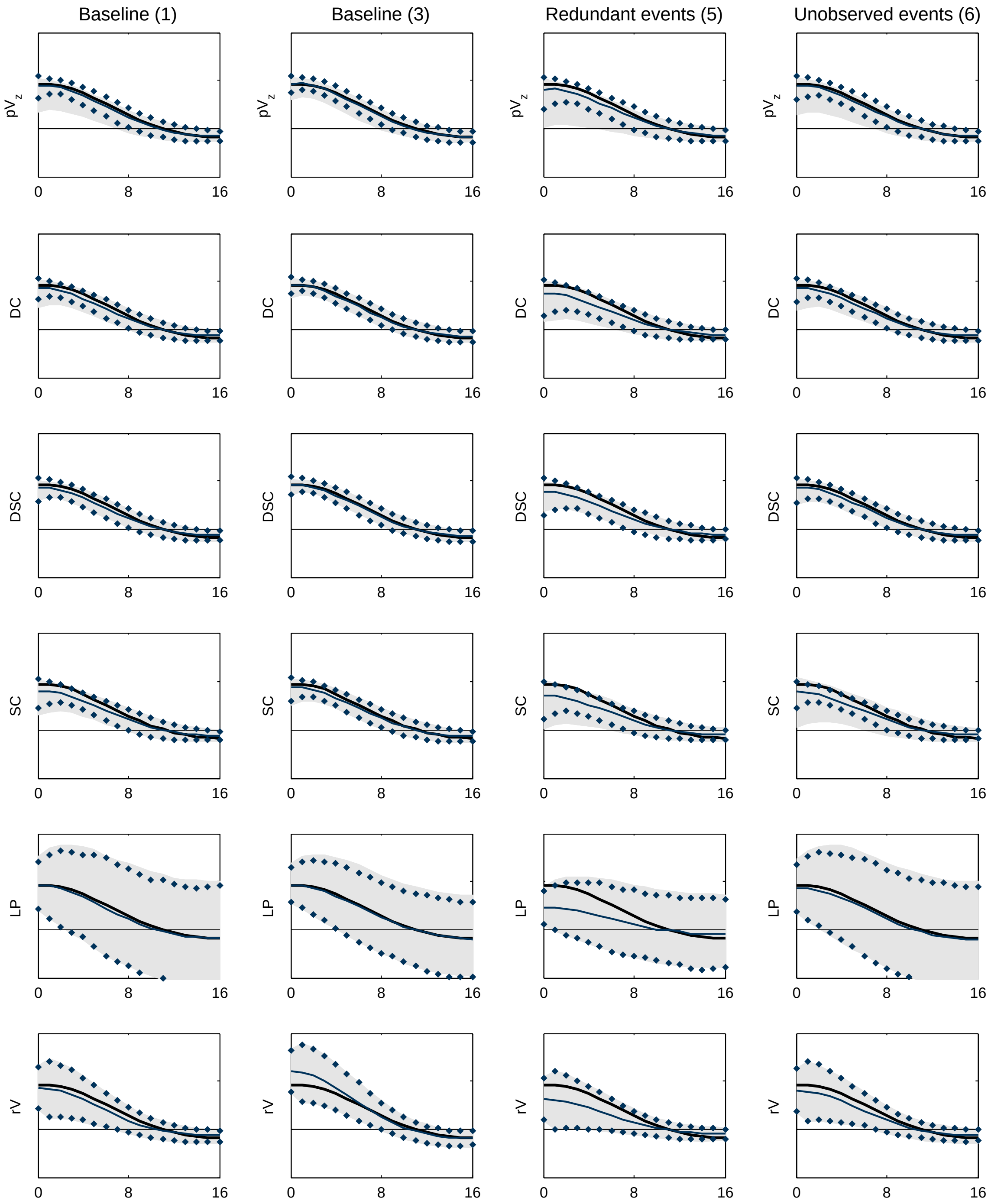
Table 1: Monte Carlo Simulations

	DGP			h = 0							h = 4	
	m	σ_θ		pV_θ	pV_z	DC	DSC	SC	LP	rV	DC	DSC
Baseline simulations												
(1)	10	0.5	Bias	-.03	-.03	-.05	-.06	-.12	-.01	-.06	-.09	-.09
			IQD	.41	.44	.43	.49	.60	.97	.88	.34	.36
			CB	.62	.70	.56	.41	.72	1.11	.92	.45	.36
(2)	10	1.0	Bias	.01	-.03	-.05	-.07	-.16	.03	-.05	-.09	-.10
			IQD	.41	.51	.53	.64	.82	1.27	1.11	.42	.47
			CB	.69	.88	.63	.45	.78	1.41	1.15	.50	.42
(3)	20	0.5	Bias	.02	.01	.01	.00	-.04	.01	.00	-.04	-.04
			IQD	.31	.34	.33	.37	.50	.72	.60	.31	.33
			CB	.40	.46	.41	.31	.54	.83	.66	.35	.32
Lagged dependency ($\omega = 0.5$)												
(4)	10	0.5	Bias	- .04	-.06	-.08	-.08	-.15	-.00	-.15	-.16	-.15
			IQD	.41	.49	.49	.54	.66	1.07	.86	.33	.36
			CB	.70	.78	.67	.47	.78	1.14	.91	.48	.39
Redundant events ($\pi_A = 1.0$)												
(5)	10	0.5	Bias		-.11	-.17	-.16	-.22	-.46	-.30	-.18	-.17
			IQD		.77	.75	.77	.77	.67	.87	.47	.46
			CB		1.00	.86	.60	.98	.81	.91	.64	.47
Unobserved events ($\pi_R = 0.5$)												
(6)	20	0.5	Bias		-.04	-.06	-.05	-.15	-.06	-.12	-.08	-.08
			IQD		.50	.50	.51	.54	1.01	.90	.34	.34
			CB		.78	.64	.52	1.02	1.12	.91	.49	.42

The table shows statistics of standardized IRFs at horizons 0 and 4, as estimated from the various models. The true values of the IRF are .92 and .78 at $h = 0$ and $h = 4$. Bias and CB are the difference of the central estimate to the true IRF and its [0.1, 0.9] confidence bounds, respectively, evaluated as the average across draws. IQD is the [0.1, 0.9] interquantile difference in the distribution of the central estimate, as a measure of its true uncertainty. m is the number of interventions, while σ_θ is the variation of policy shocks (see equation (9). The models and remaining parameters are explained in the main text. We took 1000 draws of the DGP.

The final two simulations study the role of measurement error in z_t, which amounts to a trade-off between missing relevant and including redundant policy interventions. Measurement error is a pervasive feature of macroprudential indicators. Databases differ substantially in their coverage of policy interventions due to different data sources and ambiguity in the classification of individual interventions (Budnik and Kleibl, 2018; Alam et al., 2019). Further, the effectiveness of interventions included

Figure 2: Monte Carlo Simulations



The black solid line shows the true IRF. The blue solid and dotted lines show the central estimate and its [.10, .90] quantiles as provided by the various methods. The shaded area shows the [.10, .90] quantiles of confidence bounds. See Table 1 for the definition of the simulations. The models and the calculation of central estimates and confidence bounds are explained in the main text.

within a certain category may differ widely, as interventions are typically rather diverse and some of them simply may not have been binding.

We find that adding redundant events does more harm than missing relevant events, while the relative efficiency of models SC and DSC improves in both cases, although SC overestimates confidence bounds. We take the perspective of an econometrician who faces 20 potential policy events, but is ignorant about their relevance. Simulation (5) assumes that the econometrician mistakingly adds 10 redundant events to z_t, which have no correspondence to policy shocks θ_t: we generate $m = 10$ true events and add another $\pi_A m$ random non-zero observations to z_t. Simulation (6) studies the case that the econometrician ignores 10 relevant events: we generate $m = 20$ events, but remove a share $\pi_R = 0.5$ of those events from z_t. Unobserved events have little effect on the estimates, while redundant events result in larger downward biases and higher uncertainty of the estimates. Note that the SC prior is unaffected by unobserved events by construction. In case of redundant events it insures against mis-classification of an overly high share of true events. Overall, this suggests a conservative approach to the construction of narrative indicators, while the combination of the DC regression with an SC reliability prior appears to provide some insurance against measurement error and overly wide IRF confidence bounds at longer horizons.

4 Macroprudential Policies in the Postwar U.S.

We apply our approach to narrative indicators on postwar U.S. policy interventions related to capital requirements and underwriting standards on mortgage credit.

We include seven series in our VAR: real GDP (y_t), the CPI (p_t), the effective Federal Funds Rate (r_t), the spread between the rate of return on BAA corporate bonds and the 10-year Treasury Bond (r_t^C), real total credit to the non-financial corporate sector (c_t^P) and the household sector (c_t^H), and real residential property prices (h_t). For residential property prices, we use the Shiller U.S. national home price index. The credit data are taken from the BIS, while the remaining data are from the FRED database. With the exception of the two interest rates, the series enter the VAR as quarterly log-differences. Our estimation sample ranges from 1958Q1 to 2016Q4.

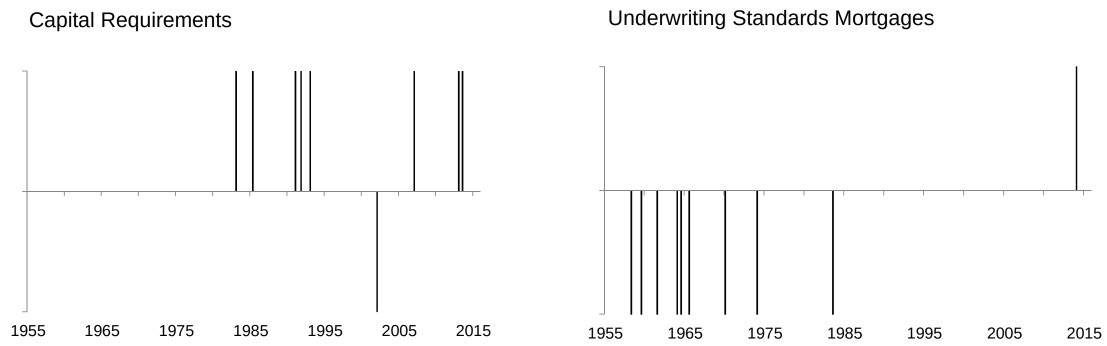
4.1 The Narrative Indicators

The major source of our information on mortgage underwriting standards is the database of Elliott, Feldberg, and Lehnert (2013), which contains a wide range of policy interventions intended to address macro-financial risks in the U.S. in between 1914 and the early 1990s. We augment the information provided by Elliot et al (2013) until the end of 2016. Further, we construct a set of policy measures related to capital requirements measures introduced after the Basel Accords and Agreements, starting with 1990, based on various sources. For the two policies we define indicators z_t such that $z_t = -1$ in case an expansionary measure was set in period t, $z_t = 1$ in case of a contractionary measure, and $z_t = 0$ otherwise. This results in 10 events for capital requirements in between 1982 and 2015, and 10 changes for underwriting standards in between 1958 and 2015. The events are listed in Annex A.5.

Capital requirements and borrower-based measures (of which underwriting standards are an important category) represent the most widely used macroprudential policy instruments since the-Basel III agreements (e.g. ESRB, 2016). A number of studies, mostly based on cross-country panel regressions have provided evidence on significant declines in credit volumes and house prices after regulatory tightenings in either type of measure.[8] Little ist yet known on the persistence of these effects, policy lags, and on the macro-economic impact of the measures (Galati and Moessner, 2016). Most closely related to our study are two recent papers that have used local projection methods to assess dynamic macroeconomic effects. Richter, Schularick, and Shim (2018) study the effects of loan-to-value limits on output and inflation for a panel of emerging economies. They find moderate declines in GDP and, surprisingly, an increase in inflation. The latter result may be due to the limited set of control variables in the regression. which lacks in particular credit and house prices. Eickmeir, Kolb, and Prieto (2018) assess capital regulation in the U.S., based on six observations of exogenous policy interventions. They report declines in GDP, inflation, and credit in response to regulatory tightenings.

[8] e.g. Kuttner and Shim (2012), Vandenbussche et al (2015), Kim and Mehrotra (2017), Cerutti, Claessens and Laeven, (2017), Budnik and Bochmann (2017), Akinci and Olmstead-Rumsey (2018).

Figure 3: The Narrative Indicators



Positive values indicate policy tightenings, while negative values indicate easings.

The exogeneity of narrative macroprudential indicators is of some concern. Elliot et al (2013) consider policy interventions of a cyclical nature, aimed at controlling macro-financial risks in response to economic conditions. However, as argued by Richter et al (2018), an immediate response of macroprudential authorities to a macro-financial shock within the same quarter seems a rather exceptional event. Instead, authorities in general would respond at some point in time to the emergence of macro-financial imbalances that have built up gradually over time. For capital requirements and underwriting standards, this is also reflected in the specificities of the U.S. institutional framework, as the responsibility for policy interventions has been distributed over various different agencies, including the U.S. Congress. Their actions have not necessarily been coordinated ex-ante. Moreover, a number of policy actions required multiple steps and consultations, which made the exact timing of policy actions less predictable (Elliot et al, 2013).[9]

At the same time, the narrative indicators can not be regarded as exogenous to past macroeconomic shocks. As discussed in section 2.3, for a proxy VAR the exogeneity requirement on lagged dependencies translates into the testable invertibility condition that the narrative indicator does not Granger-cause the variables included in the VAR. The condition ensures that the instrument is uncorrelated with past shocks and that

[9]Contemporaneous exogeneity is less likely for various other measures studied by Elliot et al (2013), such as reserve requirements, interest rate ceilings on deposits, and credit limits. These measures have been set under the sole responsibility of the Fed partly for monetary policy purposes. They do not pass the invertibility test either.

policy shocks are spanned by the VAR residuals. One interpretation of invertibility is that all relevant policy determinants are included in the VAR.

We examine lagged dependencies from ordered probit regressions of the indicators on the lags of the endogenous variables y_t included in the VAR,

$$z_{j,t} = c + \sum_{j=1}^{n} \sum_{s=1}^{4} \beta_{js} y_{j,t-s} + v_t. \tag{10}$$

The upper panel of Table 2 shows the results from likelihood ratio tests of the joint significance of coefficients related to each series. The regressions indicate lagged dependencies of the indicators on their respective main target variables only at higher lags. When considering up to four lags, we find some predictive power of credit to households and house prices for CAP and UWM interventions, respectively. Further, the corporate bond spread predicts UWM interventions. However, these effects vanish, if only the first two lags of y_t are considered indicating that the predictive power of the indicators is concentrated at higher lags. This result confirms that macroprudential policies respond to the emergence of imbalances only with a certain delay.

Table 2: Lagged Dependency and Invertibility Tests

Ordered probit							
2 lags	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
CAP	.84	1.69	0.84	1.62	1.12	4.96	4.08
UWM	1.00	1.65	*7.15	3.56	.02	1.70	1.66
4 lags	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
CAP	4.88	6.02	5.38	6.93	7.16	**13.78	4.20
UWM	3.05	2.09	7.89	*9.51	.84	3.18	*10.03
Invertibility test						Nr of events	
	1	4	8			+	−
CAP	6.23	20.29	63.71			8	1
UWM	3.90	18.78	69.65			1	9

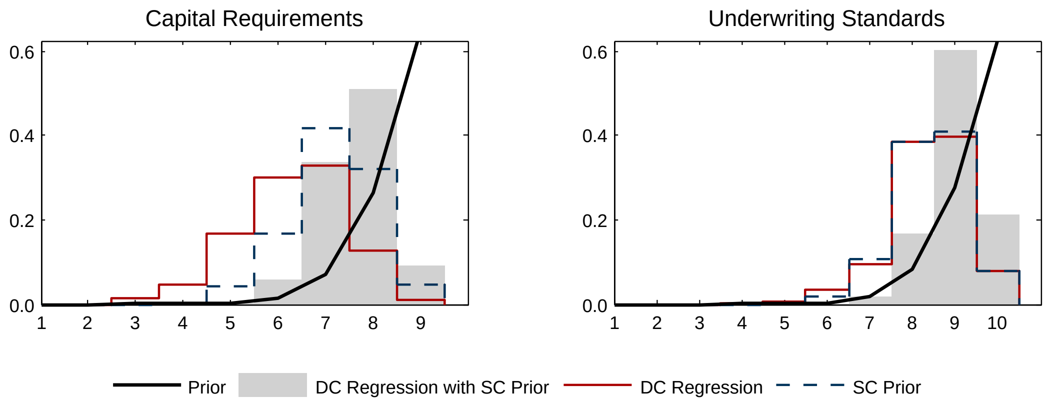
The upper panel shows the LR statistics of $\beta_{j,1} = \ldots = \beta_{j,p} = 0$ in equation (10) for values of $p = 2$ and $p = 4$. The statistics are χ^2-distributed with 2 and 4 df, respectively. '*' and '**' indicate significance at 5% and 1% levels, respectively. The invertibility test is based on VAR with 4 lags. The test statistics is χ^2-distributed with 7, 28, and 56 df for 1, 4, and 8 lags with 10% critical values of 12.02, 37.92, and 69.94.

The results of the test against non-invertibility are shown in the second panel of Table 2. The VAR includes 4 lags of the endogenous variables as suggested by the AIC and up to 8 lags of z_t. Invertibility is accepted at the 10% significance level for both indicators, suggesting that they are suitable instruments for our VAR. We remove one event from the capital requirements indicator that is correctly classified by the ordered probit. Figure 3 plots the resulting indicators.

4.2 Impulse Responses to Policy Shocks

We turn to estimating the impulse responses (IRFs) to macroprudential policy innovations from our narrative VAR. We consider three models, i.e. the DC regression, the SC prior, and the combination of the two criteria in model DSC, which gives the SC criterion an interpretation as reliability prior. We define the prior of λ as a uniform distribution over support $[0.9, 1]$. We use the seven variables described above, include eight lags and impose a standard Minnesota prior on the reduced form VAR based on a standard Normal-Wishart prior for B_+ as described by Karlsson (2013).[10]

Figure 4: Sign Concordance Posterior Densities



The shaded area shows the posterior density of the sign concordance statistics $m\varphi$ for model DSC. The lines show the same posterior density for models DC and SC and the sign concordance prior.

[10]We specify the prior variance of coefficient $B_{s,ij}$ as $\tau_{s,ij} = (\pi_0 * s^{(-\pi_3)})^2 s_j$, where s_j is the residual variance of an univarate autoregressions of series $y_{i,t}$. We set overall tightness $\pi_0 = 0.2$, lag decay $\pi_3 = 0.5$, and use a mean value of $B_{1,ii} = 0.3$ for the first own lag. For Σ we use an inverse Wishart prior $IW(S, n+2)$, where S is a diagonal matrix with elements s_i on the main diagonal. The results are based on 1000 draws from the posterior.

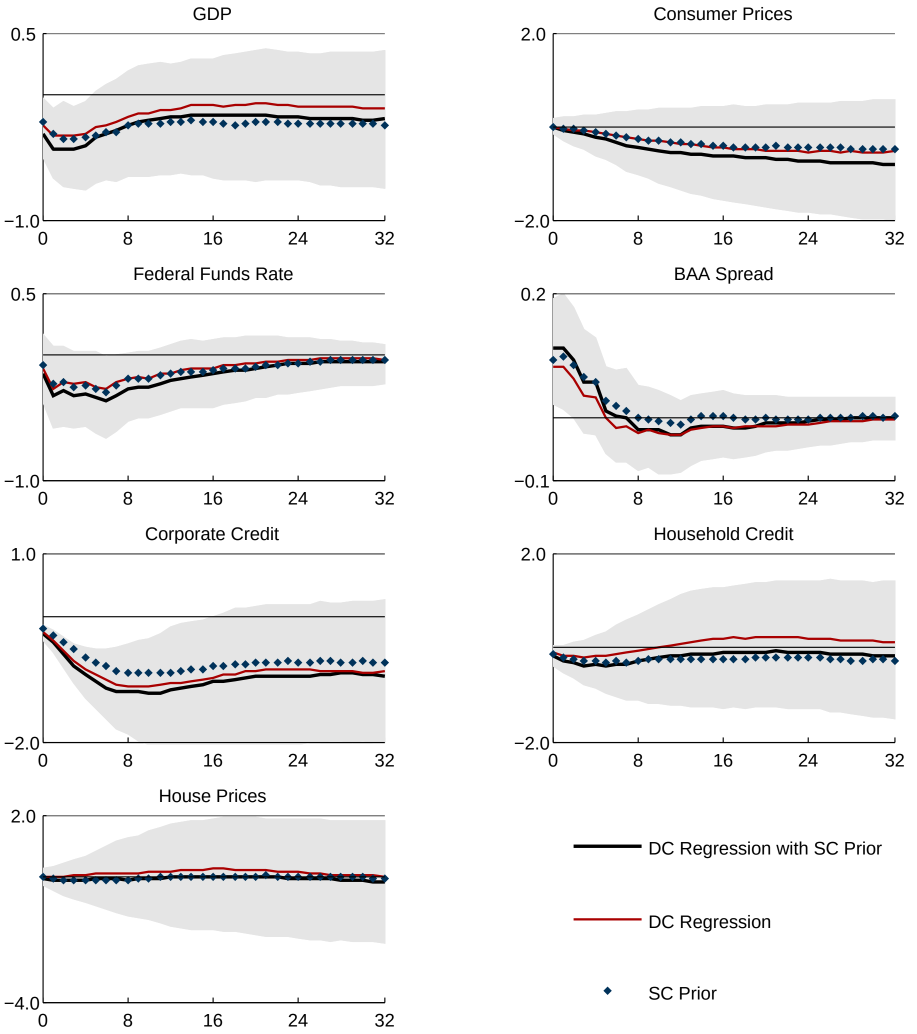
Figure 4 shows the sign concordance posteriors from the three models. For UWM measures, the number of correctly classified events peaks at values of 9 out of 10 events with little difference between DC and SC restrictions. By contrast, for CAP measures the DC restriction gives rise to a substantial share of draws with sign concordance $\varphi < 0.5$ resulting in a median value of the SC posterior of below 0.7. This indicates that our CAP instrument is somewhat weaker compared to the UWM one. The combination of the DC regression with the SC prior acts to reduce the weight of draws with low φ. For both models, this turns out to shift the SC posterior to the right not only compared to the DC, but also to the SC restriction.

Figures 5 and 6 show the impulse responses (IRFs) to policy tightenings in capital requirements (CAP) and underwriting standards (UWM), respectively. IRFs are standardized to give the response to a shock of 1%. We show results for nominal residential property prices p_t^H. The IRF estimates are very similar across the three models. For CAP measures, the DSC estimate results in slightly larger responses than the DC regression. In line with the simulation results from section 3 we also find moderately smaller median responses and larger confidence bounds from the SC prior compared to models DC and DSC (see Fig A.2 in the Annex).

The responses to CAP and UWM measures are similar and they have the expected signs. For both measures, a policy tightening induces a persistent decline in corporate credit of close to 1%, while the corporate bond spread is subject to a small, but significant increase of close to 10 basis points. At the same time, economic activity, inflation, and the Federal Funds rate decline.

However, there are also two interesting differences between the two types of policy measures. First, the impact of a change in capital requirements is concentrated on corporate credit, while household credit and house prices remain largely unaffected. By contrast, a change in underwriting standards impacts evenly on both credit categories and results in a pronounced decline in house prices. These differences suggest that a shift towards a credit portfolio subject to lower risk weights is an important element in the bank response to a change in capital requirements.

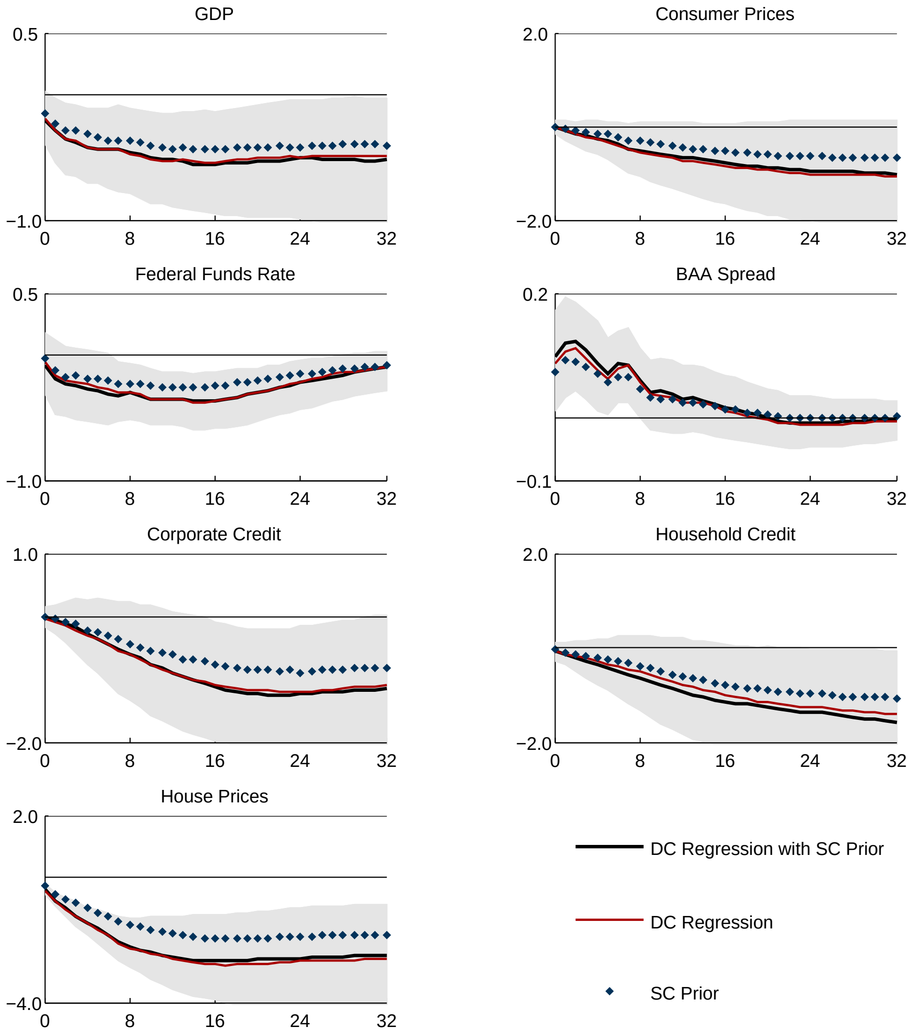
Figure 5: Standardized IRFs Capital Requirements



The graphs show the median estimates of IRFs to a shock of 1 % from models *DSC*, *DC*, and *SC*, together with [0.10, 0.90] quantiles for model *DSC*.

Second, the speed and persistence of the responses differ between the two types of measures. The effects of capital-based measures are more immediate and reach their maximum impact on corporate credit after two years. At this point, the corporate bond spread returns to baseline and the impact on economic activity wanes out. For underwriting standards, the response of corporate credit reaches its maximum after about 4 years, while household credit and nominal house prices stabilize only after even longer horizons. Similarly, the effect on economic activity and the corporate bond spread is larger and more prolonged than for capital requirements. One possible explanation for the immediate response of GDP and the Federal Funds rate to either policy measure is a tightening of credit conditions for short-term firm finance, which is subject to high risk weights, and correspondingly lower liquidity needs of banks.

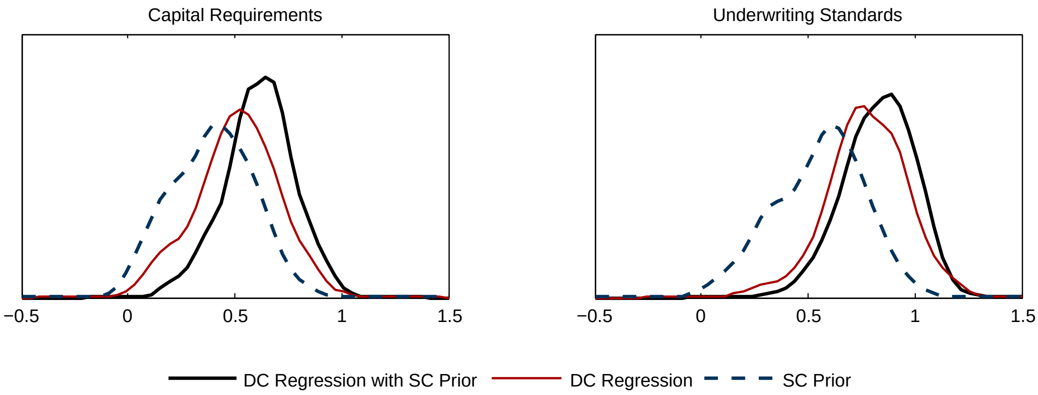
Figure 6: Standardized IRFs Underwriting Standards



The graphs show the median estimates of IRFs to a shock of 1 % from models *DSC*, *DC*, and *SC*, together with [0.10, 0.90] quantiles for model *DSC*.

Turning to estimates of the mean policy impact, Figure 7 shows the posterior distribution of parameter γ as from equation (7), while Table 3 presents the mean impact of a policy intervention on the series included in the VAR. The table shows the maximum response of each series together with its horizon. We find the impact of measures related to underwriting standards to be about twice as large as those of capital requirements. Based on estimates from model *DSC*, UWM measures on average resulted in a maximum decline in corporate and household credit of 1.0% and 1.6% after 5 and 15 years, respectively, while GDP dropped by close to 0.5%. CAP measures resulted in a decline in corporate credit of 0.8% and of GDP by about 0.3%.

Figure 7: Posterior Density of Mean Policy Impact



The plot shows the posterior distribution of parameter γ for models *DSC*, and *DC* and *SC*.

Table 3: Maximum Response to Average Policy Shock

	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
Capital requirements							
DSC	- .26	-.48	-.22	0.07	-.77	-.23	-0.25
	(1)	(54)	(6)	(1)	(10)	(3)	(50)
DC	-.16	-.24	-.13	0.04	-.50	-.10	.12
	(1)	(59)	(6)	(0)	(8)	(3)	(15)
SC	-.12	-.15	-.08	.03	-.31	-.15	-.07
	(3)	(53)	(6)	(1)	(11)	(59)	(51)
Underwriting standards							
	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
DSC	-.47	-.91	-.30	0.10	-1.04	-1.63	- 2.18
	(48)	(59)	(15)	(2)	(21)	(59)	(18)
DC	-.41	-.83	-.27	.09	-.86	-1.33	-2.14
	(58)	(59)	(15)	(2)	(19)	(58)	(51)
SC	-.25	-.39	-.15	.05	-.46	-.74	-1.13
	(54)	(57)	(15)	(2)	(23)	(53)	(39)

The table shows the maximum response of the series to the mean policy shock over a horizon of 60 quarters. The numbers are median estimates of IRFs scaled by γ for each individual draw. Numbers in brackets shows the corresponding horizon.

We estimated various alternative models to assess the robustness of our findings. The results from some of these estimates are shown in Figures A.4 and A.5 in the Annex. Figure A.4 shows estimates based on an uninformative prior for the reduced form VAR with 4 lags. We have also experimented with adding a banking deregulation index d_t as an exogenous variable to the VAR in order to control for the deregulation of the U.S. banking sector in the 1980s as documented Kroszner and Strahan (1999, 2014) and Mian et al (2017), and with using alternative measrues of credit and interest rate spreads (see Annex A.4). None of these changes our findings. Further, we experimented with different SC priors: using a tighter support of λ of $[0.95, 1]$ has little effect, while looser priors naturally lead to wider confidence bounds.

Figures Annex A.5 and A.6 show the results from alternative methods, i.e the frequentist proxy VAR, local projections, and the recursive VAR. Estimates from proxy and recursive VARs are similar to those from DSC, while confidence bounds are considerably wider. Some differences to our main estimates do arise, but they go in opposite directions. For CAP measures, the proxy VAR finds small increases in credit to households and house prices, while the recursive VAR finds significant declines. For UWM measures, the proxy VAR finds a more immediate GDP response, while the recursive VAR finds a delayed response and does not detect a decline in house prices. Local projections give rather erratic results.

Our results for capital-based measures differ from those of Eickmeir et al (2018), which are based on local projections and use monthly data from 1980 to 2008 with six narrative policy observations. The signs of the impulse responses are overall identical, but Eickmeir et al (2018) find considerably larger and less persistent responses. For instance, they estimate a decline in loan volumes of about 5 % after about 1 1/2 years with a return to baseline after 3 years. Our results are more in line with the literature. From a meta-analysis of studies based on panel regressions, Gadea-Rivas et al (2019) find an average response of credit volumes of about 0.5% in advanced economies after a year. Similarly, Galati and Moessner (2017) conclude that the effects of capital-based macroprudential policies are small in advanced economies.[11]

[11] For instance, Cerutti et al (2015) find that financial-institutions targeted macroprudential policies do not have a significant effect on credit growth in advanced economies. In a meta-analysis of studies based on bank level data, Boissay et al (2019) report larger effects from micro-based studies

5 Conclusions

One purpose of our paper was the adaptation of the Bayesian proxy VAR approach to sparse qualitative instruments. While our simulation study found proxy VARs generally to perform well, our adapation appeared to provide improved inference, as frequentist bootstrap methods tend to overestimate confidence bounds in the case of sparse instruments. Moreover, our Bayesian version lends itself more naturally to extensions such as large VARs to cope with invertibility issues or panel VARs to enhance the information on narrative indicators in a multi-country approach. Our results also indicate that regression-based approaches are less efficient than proxy VARs in the case of sparse indicators.

Our application to the effects of macroprudential policies in the postwar U.S. indicates long transmission lags and high persistence in the response of credit and house prices to policy interventions. Such delayed response has implications for counter-cyclical macroprudential policies suggesting a need for rule-based forward-looking policies. Moreover, accounting for long transmission lags appears essential for estimating the impact of policy measures. Studies based on cross-country panel regressions may understate the latter. Another policy-relevant finding is a shift to bank credit portfolios with lower risk weights in response to tighter capital requirements. In our estimates, shifts in capital requirements left household credit and house prices largely unaffected.

Our findings are also informative on the general effects of shifts in credit supply and houshold collateral constraints. The latter, in particular, are regarded as an important factor in the propagation of leverage cycles (Geanakoplos, 2009). In this respect, the persistent effects of shifts in mortgage underwriting standards underpin the respective properties of leverage cycles as documented, for instance, by Claessens et al (2012) and Rünstler and Vlekke (2018). Similarly, the results also shed some light on the most recent U.S. housing cycle, as the easing of borrowing constraints due to financial innovation has materialized in house prices only with long lags, a finding that has also been stressed by Fieldhouse et al (2017).

using quasi-experimental designs with bank-level data, but argue that substitution effects across individual banks are important and may substantially reduce effects at the aggregate level.

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Annex 1: Linear Discriminant Analysis

Consider a dichotomous variable z_t that takes the value $z_t = 1$ for m observations and $z_t = 0$ for the remaining $T - m$ observations. The objective of discriminant analysis is to estimate function $\psi(x_t)$ to predict z_t from a set of random variables $x_t = (x_{1,t}, \ldots, x_{n,t})$ based on the rule $\hat{z}_t = 1$ if $\psi(x_t) > 0$ and $\hat{z}_t = 0$ otherwise (e.g. Maddala, 2013: 79ff). $\psi(x_t)$ is chosen to minimize the objective function

$$C = C_1 \int_{R_1} f_1(x_t)dx + C_0 \int_{R_0} f_0(x_t)dx,$$

where $f_k(x_t)$ denote the conditional distributions of $x_t|z_t = k$. R_1 defines the region such that $\psi(x_t) > 0$ if $x_t \in R_1$ and R_0 is the complement of R_1. C_k is the cost of misclassifying a member of group G_k.

Under the assumption that $x_t|z_t = 1 \sim N(\mu_1, \Sigma)$ and $x_t|z_t = 0 \sim N(\mu_0, \Sigma)$, the optimal discriminant function is linear, $\psi(x_t) = \psi_1^T x_t$. Under the specific loss function $mC_1 = (T - m)C_0$, the maximum likelihood estimate of parameter vector ψ_1 maximizes the ratio of the squared difference in means between groups and the variance within groups, $(\psi_1^T \Sigma \psi_1)^{-1} \left[\psi_1^T (\mu_1 - \mu_0)^2\right]^2$. This is equivalent up to scale to estimating a via OLS from the regression $z_t^* = a_0 + a^T x_t$, where $z_t^* = z_t - m/T$. The loss function implies that the cost of misclassification is inversely proportional to the number of observations in each category. Under small m this imposes a high cost of misclassifying non-zero z_t, which we regard as a desired feature (Maddala, 2013:18ff).

While z_t can take the values 0, +1 and −1 in our application, the symmetry of the latter two cases allows for reducing the estimation problem to the dichotomous case. Formally, we consider functions $\psi^+(x_t)$ and $\psi^-(x_t)$ to discriminate the cases $z_t = +1$ and $z_t = -1$ against $z_t = 0$, respectively, and assume $\psi^+(x_t) = \psi^-(-x_t)$. We implement this by considering $\delta_t z_t = \text{abs}(z_t)$ and $\delta_t u_t$, where $\delta_t = -1$ if $z_t = -1$ and $\delta_t = 1$ otherwise. It is easily verified that the OLS regression (4) is algebraically equivalent to the regression $(\delta_t z_t) = a_0 + a^T(\delta_t u_t) + \zeta_t$.

One alternative to the DC regression is logistic regression. This is less efficient than DC if the regressors x_t are normally distributed. At the same time the DC regression has been found to be robust towards moderate deviations from normality (Maddala, 2013:82).

Annex 2: Combination with Sign and Zero Restrictions

DC and SC restrictions may be embedded in the approach of Arias et al (2018a) and thereby be combined with zero and sign restrictions on IRFs. We start with reviewing the approach of Arias et al (2018a). Consider the moving average representation of equation (3)

$$y_t = (\sum_{s=0}^{\infty} \Psi_s) A_0^{-1} c + \sum_{s=0}^{\infty} \Psi_s A_0^{-1} \varepsilon_{t-s}$$

where matrices Ψ_s are the elements of lag polynomial $\Psi(L) = B^{-1}(L)$ with $B(L) = I_n - \sum_{s=1}^{p} B_s L^s$. $\Psi(L)$ defines the (unscaled) IRF of SVAR (3). Further, define $g(A_0, \Psi(L)) = \left[\Psi_0^T, \Psi_1^T, \ldots, \Psi_s^T\right]^T A_0^{-1}$ and express zero and sign restrictions on column j of $\Psi(L)$, i.e. the IRFs to shock $\varepsilon_{j,t}$ as

$$Z_j g(A_0, \Psi(L)) e_j = 0$$

$$S_j g(A_0, \Psi(L)) e_j > 0$$

with appropriate selection matrices Z_j and S_j. Vector e_j denotes column j of identity matrix I_n.

The algorithm to generate posterior draws of $\Psi(L) A_0^{-1}$ under the above restrictions rests on the decomposition $A_0^T = A_* Q$, where $\Sigma^{-1} = A_* A_*^T$ and $Q = (q_1, \ldots, q_n)$ is an orthogonal matrix, $Q^T = Q^{-1}$. The algorithm proceeds by (i) drawing from the posterior $(B(L), \Sigma)$ to obtain $\Psi(L)$ and A_*; (ii) obtaining uninformative draws of Q that satisfy the zero restrictions; and (iii) applying an importance sampling step to account for volume changes due to zero restrictions and inspecting the validity of sign restrictions. Matrix Q is constructed in a recursive way: column q_j is obtained by drawing an $n \times 1$ vector $x_j \sim N(0, I_n)$ and deriving q_j from the Gram-Schmidt orthogonalization such that q_j is orthogonal to $(q_k, \ldots, q_{j-1})$ and to zero restrictions $Z_j g(A_* \Psi(L))$.

DC amounts to $n-1$ zero moment conditions, which uniquely define q_1. We obtain a draw of α as described in section 2.1 and find vector q_1 from $q_1 = A_*^{-1}\alpha$. *DC* restrictions may be combined with sign restrictions on shocks $\epsilon_{j,t}$ for all j and with zero restrictions for $j > 1$. The case of a higher number of instruments leads to block-diagonal restrictions as described in Arias et al (2018b). The *SC* posterior on shock $\epsilon_{1,t}$ is implemented from rejection sampling step. Hence, *SC* restrictions may be combined with sign restrictions on shocks $\epsilon_{j,t}$ for all j and zero restrictions for $j > 1$.

Annex 3: Monte Carlo Simulation Data Generating Process

We set

$$B_1 = \rho \begin{bmatrix} \cos(\omega) & \sin(\omega) \\ -\sin(\omega) & \cos(\omega) \end{bmatrix} \qquad A_0^{-1} = \begin{bmatrix} 1.0 & 0.3 \\ 0.3 & 1.0 \end{bmatrix}^{1/2} \begin{bmatrix} \cos(a) & \sin(a) \\ -\sin(a) & \cos(a) \end{bmatrix}$$

with $\rho = 0.9$, $\omega = 0.2$, and $a = \pi/4$. Matrix B_1 is subject to complex conjugate roots and generates cyclical fluctuations of length of $2\pi/\omega = 32$ quarters. Matrix A_0^{-1} is constructed from a Choleski decomposition times a rotation matrix, such that residuals $u_t = A_0^{-1}\varepsilon_t$, where $\varepsilon_t \sim N(0, 0.01 I_2)$, are subject to a correlation of close to 0.3, while the rotation matrix ensures that the IRF of $y_{t,1}$ to shock 1 has the desired shape.

To calibrate the number of policy shocks m we let $\sigma_\nu = 0.01$ and calibrate parameter $\overline{\theta}$ to achieve the desired expected number of policy interventions m. As $\text{var}(y_{i,t}) = (1-\rho^2)^{-1}\Sigma_\varepsilon$, with $\omega = 0.5$, lagged $y_{2,t-1}$ explains about 70% of the total variance of θ_t^*. The results presented are based on

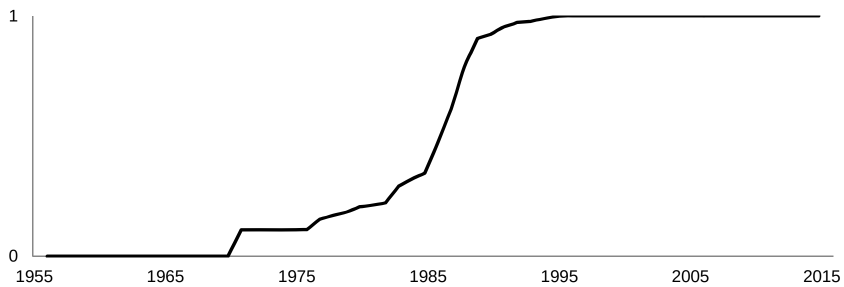
1000 draws of the DGP (9) and, for each draw of the DGP, 200 draws of the posterior or bootstrap confidence bounds, respectively. The number of observations is set to $T = 200$.

Annex 4: Deregulation Index

Our banking deregulation index is an unweighted average of two sub-indices related to inter-state and intra-state deregulation. Each sub-index takes values of zero (full regulation) to one (no regulation) with intermittent values equal to the GDP shares (as of 1980) of states, which had introduced inter-state and intra-state deregulation, respectively. Hence, the index equals zero before 1970, the beginning of deregulation, and one after 1996.

As discussed by Kroszner and Strahan (1999, 2014), deregulation was a gradual process that consolidated the fragmented banking system in multiple ways. The nature and pace of deregulation measure varied substantially across states. States differed in the timing of when they allowed banks from other states to operate in their jurisdiction and in how many other states were given access. Another source of variation was the timing of the removal of intra-state branching restrictions that prohibited banks to expand their branch network within a state.

Figure A.1: Banking Deregulation Index



We use the indices provided by Mian et al (2017), which reflect the start of a deregulation process. For example, the year of inter-state banking deregulation is defined as the first year in which a state allowed some out-of-state banks to open a branch. These decisions were state-specific and based on bilateral arrangements between states, until the Riegle-Neal Act of 1994 resulted in a general deregulation of U.S. inter-state banking. Kroszner and Strahan (1999, 2014) conclude that the process of deregulation was largely exogenous to macro-economic conditions as it was driven by a combination of technological change and shifts in private and public interest. For instance, the speed of deregulation is highly correlated with republican versus democratic state government. The index is shown in Figure A.1.

Annex 5: The Narrative Indicators

Capital Requirements

1981/15/12 Tightening

The Federal Reserve Board and the Office of the Comptroller of the Currency introduce capital standards common to all banks. The standards employ a leverage ratio of primary capital (which consisted mainly of equity and loan loss reserves) to average total assets. Standards differ slightly by type of institution with a value of of 6 % for community banks and 5 % for large regional institutions. Source: Federal Deposit Insurance Corporation (FDIC).

1983/03/01 Tightening

Congress passes the International Lending Supervision Act (ILSA). This statute directs the banking regulators to "achieve and maintain adequate capital by establishing minimum levels of capital" for banks subject to regulation. The ILSA is enacted in response to the Latin American debt crisis, which revealed a high risk of the foreign sovereign debt exposure of some U.S. banks. The law also put on firmer footing the regulators' authority to issue capital adequacy rules. Source: Federal Register.

1985/15/06 Tightening

Regulators abolish the differences in bank leverage by type of bank as established in the 1981/15/12 Act in favor of a uniform standard of 5.5 %. Banks with less than 3% of primary-capital-to-total assets are declared to be "operating in unsafe condition" and are made subject to enforcement actions. Source: FDIC.

1990/31/12 Tightening

The first stage of the Basel I rules is enacted by US regulators imposing two requirements on capital ratios, related to Tier 1 and Tier 2 capital. First, Basel I calls for a minimum ratio of total (Tier 1 plus Tier2) capital to risk-weighted assets (RWA) of 8 %, and of Tier 1 capital to risk-weighted assets of 4 %. The first stage requires respective ratios of 7.25% and 3%, while the full are phased in until the end of 1992. Source: Posner (2014).

1991/19/12 Tightening

The Federal Deposit Insurance Corporation Improvement Act categorizes institutions according to their capital ratios. Other than "well capitalized" banks (at least 10 % total risk-based, 6 % Tier 1 risk-based, and 5% leverage capital ratios) face restrictions on certain activities and are subject to mandatory or discretionary supervisory actions. Source: Government Publishing Office (GPO) .

1992/31/12 Tightening

The final implementation stage of the Basel I rules is enacted by US regulators with the own funds ratio set to 8%, and the leverage ratio set to 4%. Source: Posner (2014).

2002/01/01 Easing

The Recourse Rule reduces risk weights for AAA- and AA- rated "private-label" mortgage-backed securities (MBS) and collateralized debt obligation (CDO) tranches originated by large banks to 0.2 in line with government-sponsored enterprise (GSE)–originated MBS. For A-rated tranches, the risk weights are set to 0.5, while lower-rated tranches are assigned higher risk weights. The rule is designed to encourage securitization without encouraging risk taking, while risk weights are kept close to the 2004 Basel II risk weights. Source: Posner (2014).

2006/31/12 Tightening

The Tier 1 leverage ratio is increased to 4 %. Source: Posner (2014).

2013/01/01 Tightening

The Federal Reserve Board approves a final rule to implement changes to the market risk capital rule, which requires banking organizations with significant trading activities to adjust their capital requirements to better account for the market risks of those activities (Basel II.5). The adoption of Basel II.5, also known as the market capital risk rule, has been issued by the U.S. federal banking regulators on June 7, 2012. Source: Federal Reserve Board (FRB).

2013/30/07 Tightening

The Federal Reserve Board (FRB) introduces a supplementary leverage ratio requirement of 3% for banks using the advanced approach for RWA calculation. An additional 2% buffer requirement has been proposed for G-SIBs. Further, IRB banks are required to apply the lower of the capital ratios calculated under the standardized and IRB approaches. Source: FRB.

Mortgage Underwriting Standards

1958/01/04 Easing

Changes to requirements on loans insured by the Veteran Administration. Removal of 2% downpayment requirement on insured loans. Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first $ 13,500 of value plus 85% of next USD 2,500 plus 70% of value in excess of $ 16,000 to maximum mortgage of USD 20,000. (ii) LTV for existing construction, 90% of first US$D

13,500 of value plus 85% of next \$ 2,500 plus 70% of value in excess of \$ 16,000 to maximum mortgage of \$ 20,000. Source: Elliot et al (2013).

1959/23/09 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first \$ 13,500 of value plus 90% of next \$4,500 plus 70% of value in excess of \$18,000 to maximum mortgage of \$ 22,500. (ii) LTV for existing construction, 90% of first \$18,000 of value plus 70% of value in excess of \$18,000 to maximum mortgage of \$ 22,500. Source: Elliot et al (2013).

1961/30/06 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction set to 97% of first \$15,000 of value plus 90for existing construction, 90% of first \$20,000 of value plus 75(iii) Easing of maturity standards for new construction, maximum mortgage term raised from 30 to 35 years or 3/4 of the remaining life of improvements, whichever is less; existing construction still 30 years. Source: Elliot et al (2013).

1964/01/01 Easing

National banks are allowed to extend real estate loans with 25-year terms and 80% LTV if fully amortized. Source: Elliot et al (2013).

1964/02/09 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first \$15,000 of value plus 90% of next \$5,000 plus 75% of value in excess of \$20,000 to maximum mortgage of \$30,000. (ii) LTV for existing construction, 90% of first \$20,000 of value plus 75% of value in excess of \$20,000 to maximum mortgage of \$30,000. Source: Elliot et al (2014).

1965/10/08 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first \$15,000 of value plus 90(ii) LTV for existing construction, 90% of first \$20,000 of value plus 80% of value in excess of \$20,000 to maximum mortgage of \$30,000. Source: Elliot et al (2013)

1970/01/01 Easing

National banks are allowed to extend real estate loans with 30-year terms and 90% LTV if fully amortized. Source: Elliot et al (2013).

1974/01/01 Easing

National banks are allowed to extend real estate loans with 30-year terms and 90% LTV if 75% amortized. Source: Elliot et al (2013).

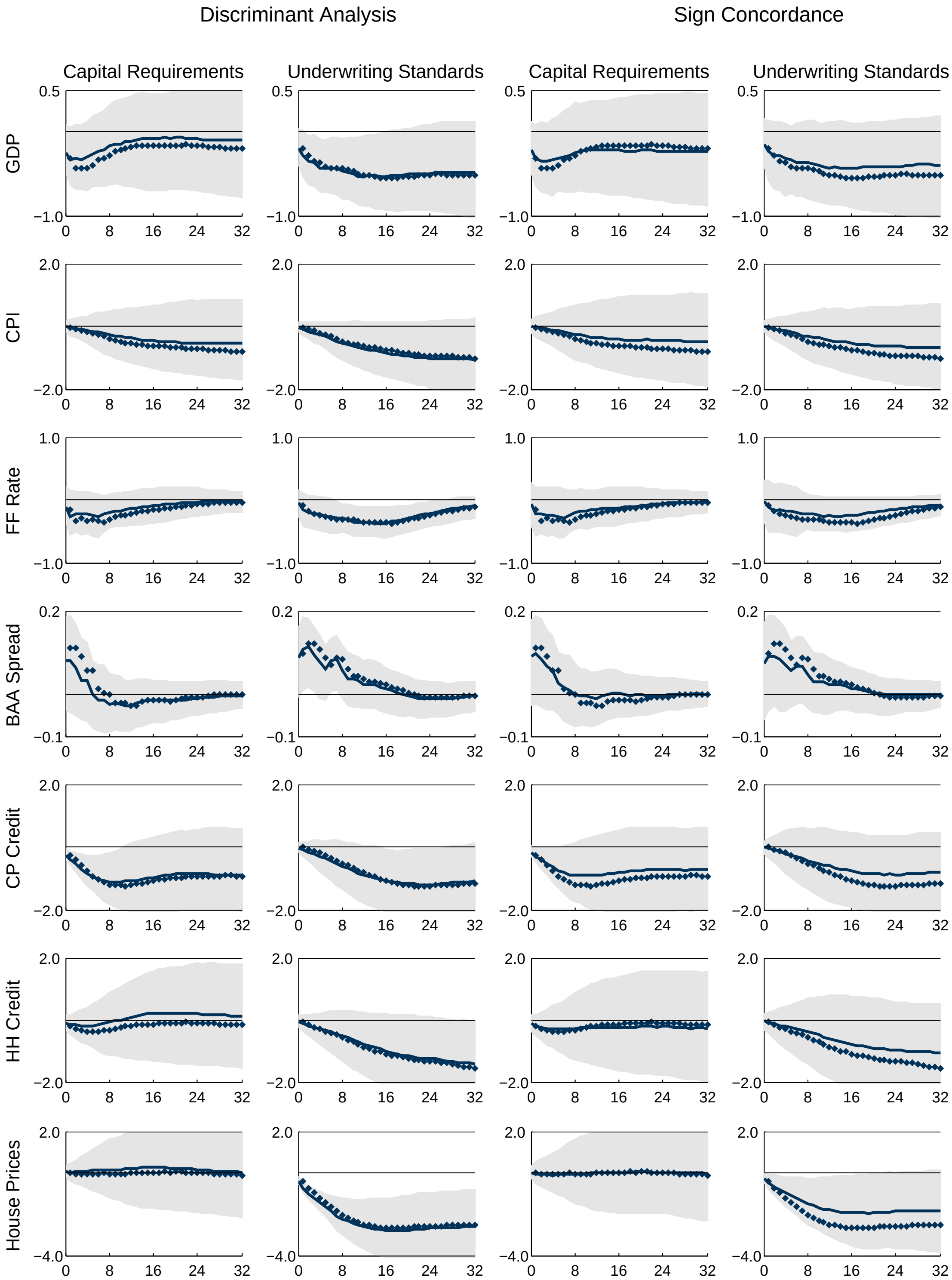
1983/01/09 Easing

LTV limits are removed for all bank mortgage loans (Garn-St Germain). Source: Elliot et al (2013).

2014/30/01 Tightening

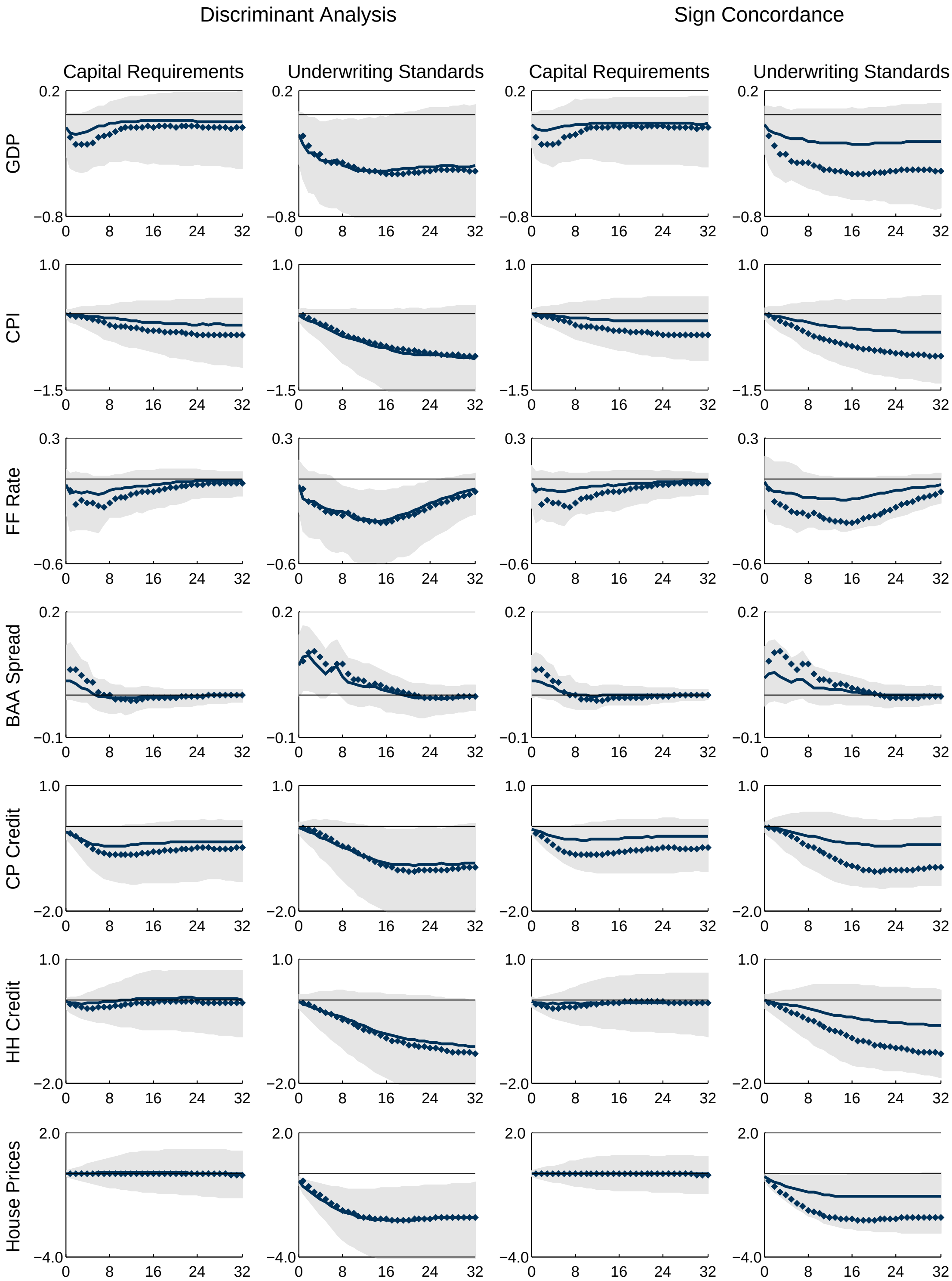
A New Ability to Repay (ATR) and Qualified Mortgage (QM) Rule by Consumer Financial Protection Bureau (CFPB) aimed at establishing a minimum set of underwriting standards in the mortgage market is established. For qualified mortgages the borrower must prove a debt service-to- income ratio no greater than 43%. Source: CFPB.

Figure A.2: Standardized IRFs for DC and SC Restrictions



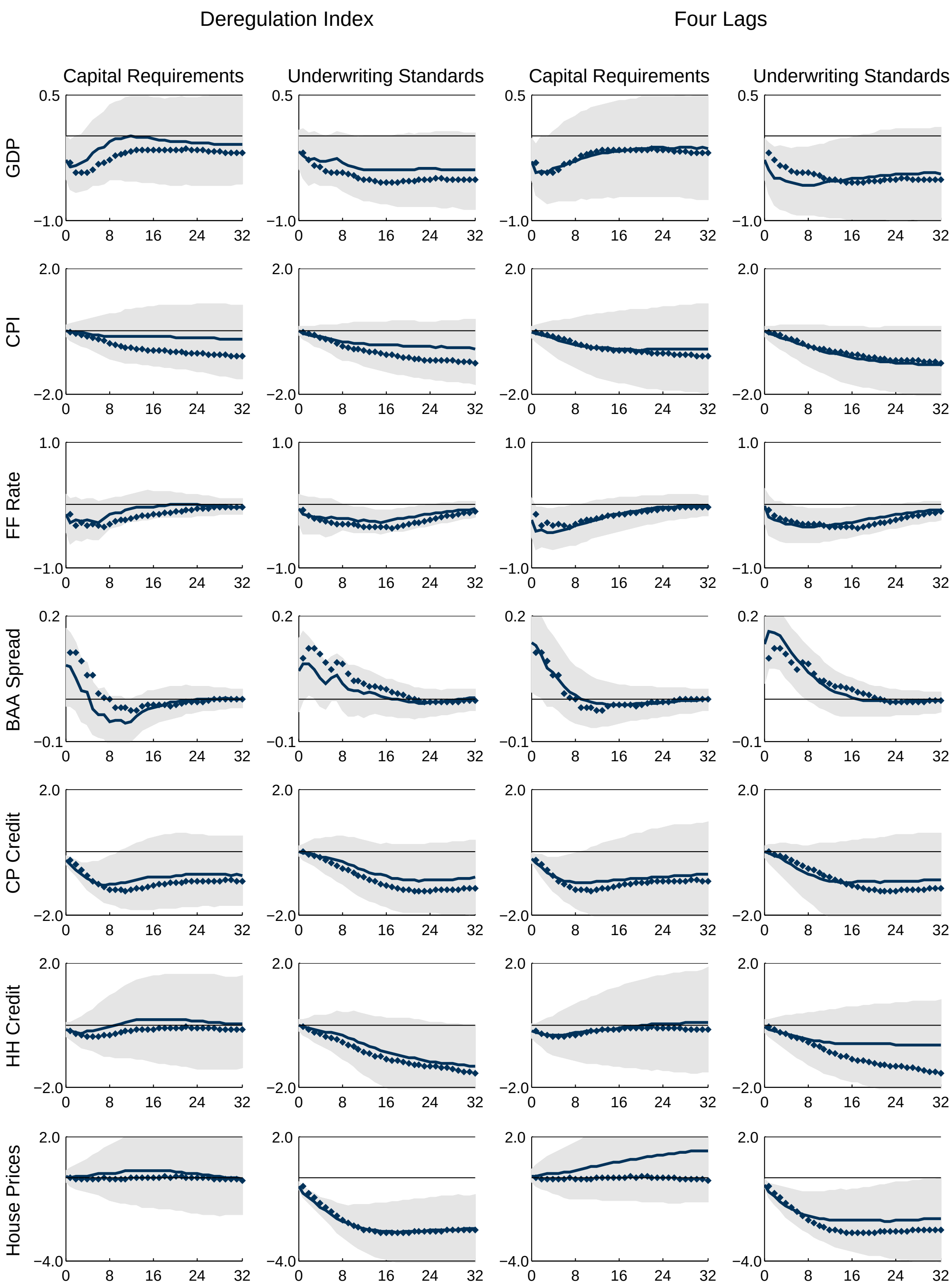
The graphs show the impulse responses to a 1% shock based on either *DC* or *SC* restrictions. The solid line shows the median and bounds show [0.1;0.9] quantiles of IRFs. The dotted line shows the main estimate from the *DSC* restriction.

Figure A.3: IRFs Scaled by the Average Policy Impact



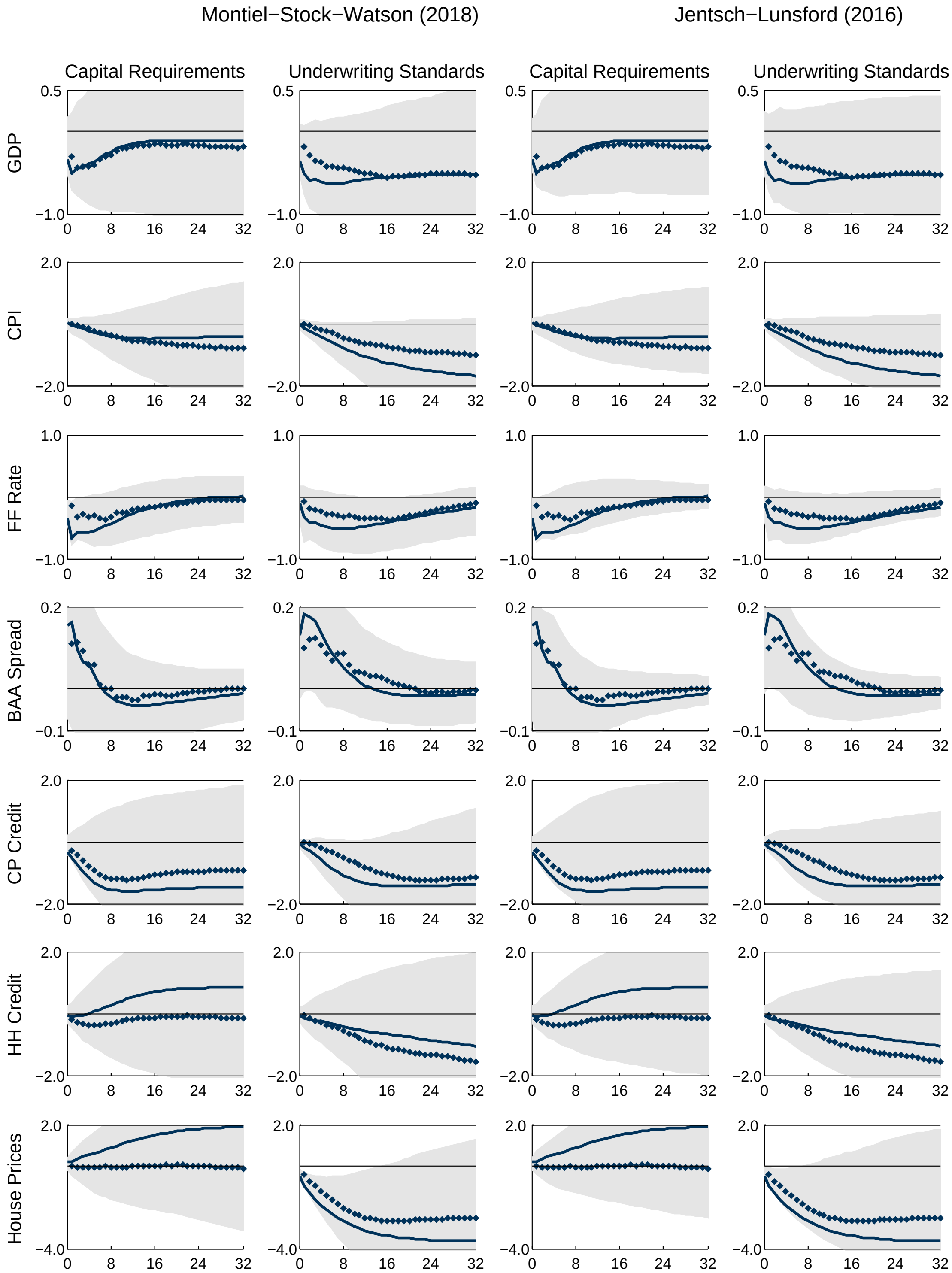
The graphs show the IRFs scaled the impact of average policy shock of size γ based on either DC or SC restrictions. The solid line shows the median and bounds show [0.1;0.9] quantiles of IRFs. The dotted line shows the main estimate from the DSC restriction. The rescaling is done for each individual draw based from the corresponding γ.

Figure A.4: Standardized IRFs from Alternative Estimates



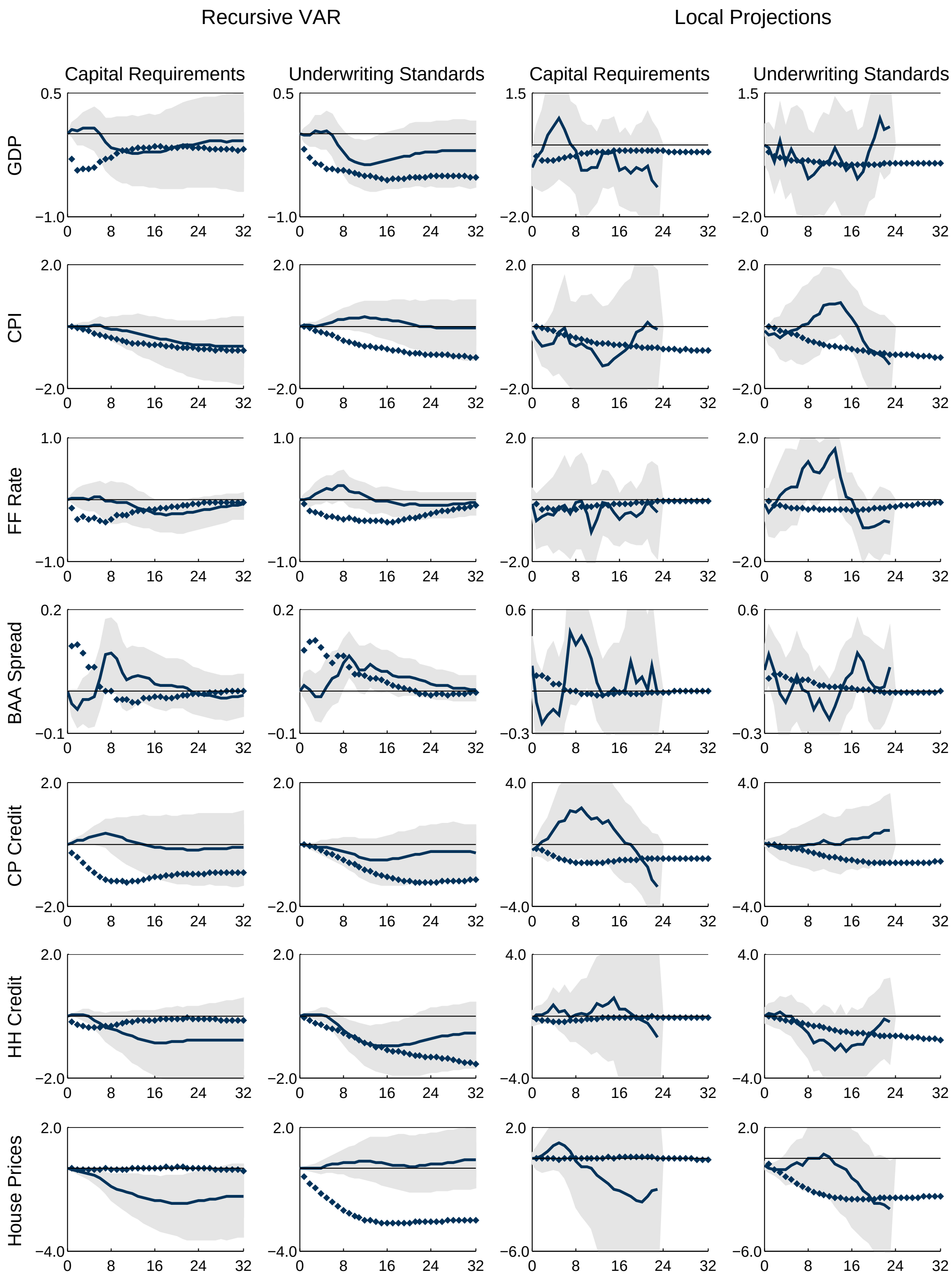
The graphs show the impulse responses to a 1% shock based on restriction DSC. The left hand graph shows estimates including the deregulation index. The right hand graph shows estimates from a VAR including 4 lags. The solid line shows the median and bounds show [0.1;0.9] quantiles of IRFs. The dotted line shows the main estimate from the DSC restriction.

Figure A.5: Standardized IRFs from Proxy VAR



The graphs show impulse responses from the bootstrap proxy VARs by Montiel Olea, Stock, and Watson (2018) and Jentsch and Lunsford (2016). Estimates are based on the code of Mertens and Montiel Olea (2018). Solid lines show the OLS estimate and bounds show [0.1;0.9] quantiles of IRFs. The dotted line shows the main estimate from the DSC restriction.

Figure A.6: Impulse Responses from LP and Recursive VAR



The graphs show impulse responses from from local projections, based on the code of Mertens and Montiel Olea (2018), and from the recursive VAR by Plagborg-Møller and Wolf (2019) with standard bootstrap confidence bounds (Lütkepohl, 2000). bounds show [0.1;0.9] quantiles of IRFs. The dotted line shows the main estimate from the *DSC* restriction.

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